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LORD LINDSAY, M.P., President, in the Chair.

John E. Gore, Esq., C.E., M.R.I.A., Asst. Engineer, D.P.W., India, Dromard Rectory, Co. Sligo;

The Rev. Frederic John Hall, Haileybury College, Hertford;

William Hobson, Esq., Park View, Whalley Range, Manchester;

Lieut. Robert Hoggan, R.N., H.M. Dockyard, Sheerness;

William C. Otter, Esq., 60 Grosvenor Park, Camberwell; and

F. H. Stevens, Esq., B.A., Clifton College, Clifton;

were balloted for and duly elected Fellows of the Society.

Letter received by Lord Lindsay from Dr. W. Foerster.

MY LORD,—As the time is approaching when the Annual Report of the Council of the Royal Astronomical Society is published, I beg to call your attention to a passage in the last Report (February 1877, p. 188), where the planetary work of the *Berliner Astron. Jahrbuch* is mentioned in a way which does not do full justice to it.

It is said in this passage that space has only been found in the *Berliner Astron. Jahrbuch* for 50 out of 125 planets which will be in opposition between January 1 and December 31, 1877, "and it is regretted that so few of the older and larger planets are included in the selection."

From these expressions it will be inferred that the *Berliner Jahrbuch* has been liable to certain external and, as it seems by the words "space has not been found," to economical restrictions which have not permitted the superintendents to do the necessary planetary work required by science.

But in every volume of the *Jahrbuch* since the year 1874, and finally in *Circular* No. 44, we have endeavoured to show by general deductions, accompanied with more and more detailed practical propositions, which have nowhere been seriously refuted and which have been approved by several meetings of astronomers, that the most perfect and exhausting care of the small planets is of prominent scientific interest, and that in the actual state of things this task can only be reasonably fulfilled by the introduction of some restrictions of a scientific nature, but in no way of economical or any other external character.

The intensity of planetary work at Berlin and in the whole astronomical world has not weakened, but has sensibly increased, since these restrictions have been carried into practice. Most astronomers concerned in that department of science have now gained the hope that, by reasonable organisation, and discernment of the essential from the secondary or imaginary wants of science, the whole planetary work in future may be fully mastered, and that thus, by an exhaustive treatment according to our propositions, more and more important results will be furnished for the small planets. It will be seen by the Royal Astronomical Society—especially from the periodic *Circulars* of the *Berliner Jahrbuch*, which now form an essential completion of the annual volume of this publication—that the Superintendency of the *Jahrbuch*, mainly conducted by my friend, Dr. Tietjen, endeavours to do the best possible to aid observers and computers of small planets.

I hope that you will be able to procure for this communication a place in the next publication of the Royal Astronomical Society.

I am, my Lord, with the greatest respect, your obedient servant,

W. Foerster,
Director of the Berlin Observatory.

Berlin,
1878, *February* 10.

Galileo's Trial before the Inquisition in the light of recent Researches.

By Sedley Taylor, M.A., late Fellow of Trinity College, Cambridge.

(*Communicated by J. W. L. Glaisher, Esq.*)

Within the last ten years a large number of previously inaccessible official documents relating to the trial of Galileo before the Inquisition have been made public. The fresh evidence thus far produced has given rise to an entirely new view as to the nature of that proceeding, a short account of which will be found in the following Paper:—

Up to the year 1867, the best informed writers held, almost

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to a man, that the Inquisition, whatever might be thought of the wisdom of its proceedings, had a clear legal right to punish the great astronomer for the course which he pursued. The text of the sentence against him stated that he had committed a palpable contempt of court by writing in favour of the Copernican doctrine in 1632 after having in 1616 been formally inhibited by the Inquisition from doing so. As long as this assertion was accepted it was obviously impossible to clear Galileo of the charge of contumacy, by which his condemnation was in the last resort determined. It is a singular circumstance that historians, many of whom were by no means antecedently inclined to repose a childlike faith in the veracity of the Inquisition, did, one and all, accept this its unsupported assertion, and with equal unanimity overlooked a glaring contradiction on the surface of the sentence, which it was reserved for a German writer to point out in 1870.

The view that Galileo was legitimately punished for contempt of court received much support in 1850 from a work published by Marino Marini, Prefect of the Secret Archives of the Holy See, and entitled *Galileo e l' Inquisizione.* It was previously known that a contemporaneous record of the trial, drawn up from day to day by the Notary of the Inquisition, was preserved among its archives: Marini, the official guardian of this record, now for the first time printed a considerable number of extracts from its contents. The object of this writer manifestly was to represent the conduct of the court as conspicuously mild and benignant; that of the accused as intolerably troublesome and rebellious. By a system of really masterly suppression and garbling of the documents to which he alone had access, Marini succeeded in completely misleading historians of the trial for more than a quarter of a century, until, in fact, the publication *in extenso* of the documents manipulated by him exposed his manipulations to the light of day.

The first tolerably complete edition of the trial-record was published in 1867 by *M. de l'Epinois*, to whose latest exertions a few months ago we owe a verbatim issue of the whole. Professor Berti also contributed an edition in 1876, and Herr v. Gebler closes the series with one still more recent, and aiming at even more minute reproduction of the original than its predecessors. The evidence supplied by the official minutes of the trial is therefore henceforth universally accessible except in so far as the genuineness of parts of the manuscript itself may be called in question.

The writer who has made the most important use of these new materials is Dr. Emil Wohlwill, of Hamburg, who published, in 1870, a short work on the trial, and followed it, late in 1877, by a detailed examination of the question whether the prisoner was submitted to the torture during the course of the proceedings. The earlier of these publications investigates an issue of much greater, indeed of cardinal, importance, viz.

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whether the Inquisition told the truth when it asserted that Galileo had, in 1616, been formally served with an order of court inhibiting him from defending the Copernican doctrine. Wohlwill maintains that this asserted inhibition had no actual existence, and that the whole incident was a fraudulent fabrication perpetrated in order to secure a condemnation which could not be compassed by legitimate means. That a forgery was committed in one of the documents of the trial-record for the purpose of carrying out this nefarious scheme, and that highly placed officials of the Inquisition were accomplices in the conspiracy, are corollaries which follow immediately from the above position.

Before stating the chief arguments on which the German writer relies, it will be well to premise a concise *résumé* of the essential facts in Galileo's case.

It is probable that about the year 1592 he became a convert to the heliocentric system, which had been propounded half a century before by Copernicus in the *De Revolutionibus Orbium Cœlestium.* He seems, however, for a considerable time after this change of view, to have continued publicly to teach the Ptolemaic system, and this, as he himself explained in a letter to Kepler, "on account of the huge number of fools" and the derision and molestation which adherents of the new doctrine would be sure to experience at their hands. But by slow degrees he must have allowed his genuine convictions to appear, for in 1615 we find him furiously attacked from the pulpit for his adherence to the new astronomy. The preacher, a monk at Florence, denounced mathematics as a diabolical art, and sought to enlist the force of ridicule on his side of the question by quoting the verse at the opening of the Acts of the Apostles—

> Ye men of Galilee, why stand ye gazing up into heaven?

as a punning hit against the star-gazing disciples of Galileo. It ought, perhaps, in justice to this monastic punster, to be remarked that his joke sounds decidedly better in the Latin of the Vulgate than it does in the English of the Authorised Version. "Galilei," the Latin equivalent for "Galileans," is identical with the proper surname of the philosopher, whose complete designation was Galileo dei Galilei. By some odd freak of custom we in England invariably call him by his Christian name only.

The enemies of our astronomer unfortunately did not confine their mode of warfare to sermons and Biblical puns—an artillery whose fire he no doubt was prepared to sustain with considerable equanimity. The monks of the monastery of St. Mark concocted a letter to the Inquisition at Rome, in which they denounced to that formidable tribunal some opinions which Galileo really did hold, and a great many more which he did not. The Holy Office, once put on the scent of heresy, set to work with its usual keenness, and before long Galileo received a private hint from friends in Rome that some underground machination was going on

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against the new astronomy. On this intimation he went up to the capital, in the hope that, by pointing out to the authorities the futility of attempting to arrest the progress of the new views by ecclesiastical censures, he might save the rulers of the Church from the humiliation of publishing decrees against the laws of nature. Galileo overestimated his powers of persuasion. The absurdity which he strove to avert was resolved upon, and the formal decree, declaring the Copernican doctrine to be false and contrary to Scripture, was issued by the Congregation of the Index on the 5th of March 1616.

It is very generally supposed that by this decree the work of Copernicus was prohibited, but this is a misconception. The *De Revolutionibus* was only ordered to be "suspended until corrected," i.e. withdrawn from circulation for the purpose of having certain alterations made in its text under the direction of the Congregation of the Index. The list of corrections subject to which the work was permitted to reappear was published in 1620; it is very short, covering, perhaps, two pages out of two hundred. The object aimed at by the correctors was manifestly the introduction of *hypothetical* language where the author had spoken *categorically*. Much skill is shown in giving this turn to a number of passages by the slightest verbal alterations, and it is only a very few paragraphs which, as not susceptible of such treatment, were ordered to be expunged. The fact that the treatise of Copernicus was allowed to reappear with only these superficial readjustments, which left its long chain of reasoning untouched in a single essential link, shows conclusively that the Congregation, though determined to get rid at a single stroke of the vexatious discussions likely to arise about the relations between Scripture and the new astronomy, did not demand more than a certain decorum of overt expression from writers who discussed the question from a purely scientific point of view.

In 1632 Galileo published his Dialogues on the Ptolemaic and Copernican systems, which led directly to all the misfortunes and sufferings of his remaining years. The work was suspended about seven months after its appearance by a peremptory order from Rome, and two months later its author received a citation to appear before the Supreme Tribunal of the Inquisition. The result of the subsequent trial is well known. The accused was declared to have rendered himself vehemently suspected of heresy, compelled to abjure, and condemned to incarceration during the pleasure of the Court: a punishment which was, however, promptly commuted to practical imprisonment in his own remotely situated villa at Arcetri, where he dragged out his remaining years under the humiliating *surveillance* and repeated menaces of the Holy Office. The main charges on which the prisoner was convicted were the following:—

(1) That the Dialogues transgressed the limits laid down by the decree of the Index.

(2) That they were in violation of a private personal in-

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> junction, containing restrictions far more stringent than those of the public decree, which the tribunal asserted to have been formally delivered to Galileo on February 26, 1616, by the then acting Commissary-General of the Holy Office.

To the first allegation the prisoner had a complete answer. He had submitted his work in manuscript to the Papal licensing authority, and obtained the regular *Imprimatur* before proceeding to publication, and therefore could not be made criminally responsible for its contents, inasmuch as it was manifestly not his business, but that of the licenser, to know what was and what was not compatible with the decree of the Index.

The second article of charge depends, of course, on the exact terms of the notice given to Galileo on February 26, 1616. The nature of the whole asserted proceeding appears from the following entries, translated from the Latin of the trial-record :—*

"Thursday, Feb. 25, 1616.—The Most Illustrious Lord Cardinal Millinus gave notice to the Rev. fathers, the Lords Assessor, and Commissary of the Holy Office that, on the censure of the Fathers Theologians passed upon the propositions of Galileo—that the Sun is the centre of the universe and locally motionless, and that the Earth moves even with a daily motion—being reported, the Most Holy ordered that the Most Illustrious Lord Cardinal Bellarmine should summon to his presence the said Galileo, and admonish him to abandon the said opinion, and that, if he should refuse to obey, the Father Commissary, in the presence of a Notary and Witnesses, should deliver to him a precept entirely to abstain from teaching or defending such doctrine or opinion, or from treating thereof; but that if he did not acquiesce he was to be imprisoned.

"Friday, Feb. 26 of same year.—In the Palace habitually occupied by the said Most Illustrious Lord Cardinal Bellarmine, and in the apartments of his Most Illustrious Lordship the above-named Galileo having been summoned, and himself being in the presence of his Most Illustrious Lordship, and in the presence of the very Rev. father Brother Michael Angelo Seghitius of Lodi, of the Dominican order, Commissary-general of the holy office, the Same Most Illustrious Lord Cardinal admonished the above-named Galileo concerning the error of the above-named opinion, and to abandon it ; and thereupon and forthwith, in the presence of myself and Witnesses, and the same Most Illustrious Lord Cardinal being likewise still present, the *Father Commissary enjoined upon and ordered the aforesaid Galileo, he being still in the same place and under formal Presentment, in the name of Our Most Holy Lord Pope and of the whole Congregation of the holy office, entirely to abandon the above-named opinion that the Sun is the centre of the universe and motion-*

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* The paragraphs thrown into italics are those which Wohlwill considers to be a subsequent and fraudulent addition.

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*less and that the Earth moves, and in Future not to hold, teach, or defend it in any manner whatever, by word of mouth or in writing, failing which proceedings would be taken against him in the Holy office; to which precept the same Galileo submitted and promised obedience.—Done at Rome as above, there being present at the same place R. D. Badinus Nores of Nicosia in the Kingdom of Cyprus, and Augustinus Mongardus, of a place in the Abbacy of Rose, in the diocese of Montepulciano, servants of the said Most Illustrious Lord Cardinal, as Witnesses."**

The above minutes are quoted verbatim in the sentence of 1633, as first-hand evidence of what occurred in 1616, and the whole incident of the Commissary's inhibition rests on no other authority. Everything therefore turns on the question whether this incident was a fact or a fiction. Wohlwill's reasons for adopting the latter of these alternatives shall next be summarily stated.

An unsuspecting reader might easily take the minute of February 26 as describing a straightforward carrying out of the order laid down in that of February 25. It can, however, be so regarded only by making an arbitrary assumption which will presently be shown to be contradicted by positive evidence. The Papal order contained in the first minute provides that the Commissary was only to intervene officially on the occurrence of a particular contingency, viz. on Galileo's refusing to obey Bellarmine's admonition. The second minute represents the Commissary as delivering his "precept" immediately upon the conclusion of the Cardinal's address, and, as the language used seems to imply, without even waiting to ascertain whether Galileo refused to obey or not. It is impossible to suppose that the Commissary would have dared, in the presence of the leading Cardinal of the Holy Office, thus to set at nought the direct instructions of the Pope himself. If therefore the two minutes are to be reconciled at all, this can only be done by assuming, without a tittle of evidence, that Galileo, when admonished by the Cardinal, refused to submit, and so brought down upon himself the Commissary's precept.

But this assumption is put out of court by a piece of direct evidence which came to light under singularly interesting circumstances. In 1848, when the suspension of the temporal power at Rome placed the secret archives of the Vatican for a brief interval within the grasp of the revolutionary government, Professor Silvio Gherardi, one of the Secretaries of State, seized the opportunity to institute a search among the records of the Inquisition with a special eye to documents bearing on the case of Galileo. After an extremely laborious exploration he succeeded in unearthing a considerable number, which, as subsequent comparison has shown, were not included in the collection since published, and probably formed part of an entirely distinct series.

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* The text of *v. Gebler's* edition has been followed.

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One of these documents relates how, on March 3, 1616, Cardinal Bellarmine reported as follows to the Pope:—"Galileo Galilei, mathematician, admonished by order of the Sacred Congregation to abandon the opinion which he has hitherto held, that the Sun is the centre of the spheres and immovable, but the Earth movable, acquiesced."

No words are needed to enforce the decisive bearing of this pregnant sentence. It is evident that Bellarmine is here reporting his execution of the order contained in the minute of February 25, and its successful result. Galileo had acquiesced under his admonition, and the contingency which was to entail the Commissary's intervention had not arisen. The attempt to reconcile the conflicting minutes by assuming that Galileo refused to submit must therefore be abandoned. It will be noticed how Bellarmine's report corroborates the minute of February 25 and the earlier part of that of February 26, while it invalidates the latter's disputed close.

Singularly enough, the great Cardinal has left behind him a further piece of testimony equally destructive of the *rôle* attributed to the Commissary. Having been applied to by Galileo, a few months after the interview, for a memorandum stating exactly what had occurred, Bellarmine forwarded an autograph certificate that only the declaration published by the Index Congregation (i.e. their decree of March 5 on the Copernican theory) had been announced to Galileo. It is manifest that this statement entirely excludes the delivery to him of a further and infinitely more stringent prohibition, such as the Commissary's precept. The Court in their sentence quoted Bellarmine's certificate as genuine and authentic, and by so doing implicitly invalidated the asserted action of the Commissary, on which, nevertheless, they persisted in condemning the prisoner. It is this flagrant contradiction between two pieces of evidence, each insisted on by the Inquisition as trustworthy, to which reference has already been made as having been for the first time pointed out by Wohlwill.

The evidence given by Galileo during his examinations by the officials of the Holy Office, and the statements of his written defence handed in to the tribunal, minutely confirm the position that no such prohibitory order as the precept of 1616 had ever reached him. In the interrogatory of April 12, 1633, the efforts of the examining official were specially directed towards eliciting an admission of the delivery on February 26, 1616, of the Commissary's precept. The examiner manifestly held in his hand the minute the genuineness of which is now called in question, and sought, by reading aloud portions of its text, to recall the incident it described to the memory of the accused. His attempts were fruitless. Galileo, though plainly anxious to show the utmost docility to the wishes of the Court, and willing to accept the technical term "precept" ("*precetto*"), by which the examiner designated the communication made to him, asserted that he had no recollection of the precept in question having

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been delivered to him by any other person than Cardinal Bellarmine.

In his defence, presented to the Court on May 10, Galileo remarks that the authentic attestation of what took place on February 26, 1616, which he possessed in Bellarmine's certificate, did not contain a trace of an indication that any injunction had been laid upon him save that general pronouncement which concerned everyone alike—viz., of course, the decree of the Index.

These statements appear to be supported by the strongest internal probabilities. We may at once set aside the notion that Galileo may have *forgotten* the Commissary and his message. His detailed statements on the multifarious subjects as to which he was examined show that he possessed a memory of unusual tenacity. When we think what in Italy, in the seventeenth century, it was in the power of the Inquisition to inflict on those who incurred its displeasure, we cannot doubt that, had Galileo been actually brought face to face with a leading official of the dreaded tribunal, and received from him the order of the Court, ending with the warning "Obey, or a suit will commence against thee in the Holy Office," the scene must have been branded into his memory in letters never to be effaced while life and memory endured.

It may indeed be urged, and has been urged, that Galileo, though conscious that he had received the Commissary's precept, chose deliberately to deny it. Now as, unfortunately, it cannot be denied that, at a later point of his examination, Galileo was betrayed into asserting on oath what was unquestionably false, there is nothing antecedently improbable in the suggestion that he may be doing the same thing here. Nevertheless there are, I think, indications which point decisively to an opposite conclusion.

We may assume that a sensible man, to say nothing of one of eminent tact and acuteness, will not, however unscrupulous he may be, tell a deliberate lie by which little or nothing is to be gained, especially if by doing so he is likely to expose himself to serious and imminent peril. Now Galileo, before advancing the statements which we are discussing, was made aware by the official who examined him that the Court was in possession of a formally attested record of what took place on February 26, 1616. He must have felt, therefore, that it was useless to deny what thus admitted of direct legal proof—and not merely useless, but excessively dangerous; it being the practice of the Inquisition to resort to the application of the torture when there was reason to suspect that a witness was not telling the truth. For these reasons, if Galileo had been conscious of having really received the Commissary's precept, he would surely not, in the face of the official minute which he saw in his interrogator's hand, have ventured on the desperate course of denying all knowledge of it.

There is a further argument to which considerable weight ought

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to attach. A person who reads through the whole of Galileo's answers on April 12 will hardly fail to reach the conclusion that he did not really know what his examiner meant by the "precept," and fancied he alluded to a particular form of words employed by Cardinal Bellarmine, and that the question was whether or not certain crucial expressions, such as "to *teach*" and "in *any* manner," were or were not included. On the whole, the balance of probability inclines very decidedly in favour of the truthfulness of Galileo's asseverations. They contain, therefore, strong evidence for the non-delivery of the precept of 1616.

In addition to the direct evidence which has been already detailed, Wohlwill has collected a considerable body of collateral indications which tend to exclude the asserted action of the Commissary. Some of these shall next be briefly stated.

The fact that Galileo was a devout and warmly attached member of the Roman Church affords some antecedent presumption that he would not have refused to yield to an admonition from her greatest Cardinal, with whom too he stood on a footing of private friendship. But further, in his private letters he is found repeatedly congratulating himself on the docile attitude he had assumed towards the Church on the Copernican question. Thus, in a letter written on March 6, 1616, he says that his conduct throughout the whole transaction had been such that "no saint could have borne himself with greater reverence or greater zeal towards Holy Church." This is hardly the language of a man who but ten days before had met Bellarmine's admonition with a refusal to acquiesce.

The same asseverations reappear in a letter written some two years after his condemnation, when all hopes of softening the hearts of his persecutors by professions of zeal for the Church must, if he ever entertained them, have utterly vanished :—

"I have said," he writes, "that I do not hope for any alleviation, and this because I have committed no crime whatever: I might have hoped to obtain grace and pardon had I erred; for transgressions afford to princes the means of exercising mercy and indulgence; but in the case of a man unjustly condemned, the rigour of the sentence must be sustained in order to keep up an appearance of its having been legitimately arrived at. This afflicts me less than others may think, because two consolations constantly support me. One of these is that there is not to be found in all my writings the least shadow of lack of piety or of reverence for Holy Church. The other is my own conscience, fully known only to myself on earth and to God in heaven, who well knows that in the cause for which I suffer many might have acted and spoken with greater learning, but not one, even of the holy fathers, more piously nor with greater zeal towards Holy Church, nor, in short, with a more holy intention, than I."

From a letter of Galileo, dated March 12, 1616, we learn that on the previous day he had a most gracious audience of the

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Pope, who, in reply to some expressions of alarm let fall by Galileo at the prospect of being the object of malignant and implacable attacks, assured him that he was so well thought of by himself and by the whole Congregation that credit would not be lightly given to the persecutors.

If on February 26 Galileo had met the admonition of Bellarmine with a refusal to submit, and had thereupon been visited by the Commissary, "in the name of our lord Pope and of the whole Congregation of the Holy Office," with a peremptory and threatening inhibition, it is all but inconceivable that the Pope should, within a fortnight, have assured him how well he stood, "both with himself and the whole Congregation."

Galileo's subsequent conduct was moulded most carefully in conformity to the Index decree, but was quite inconsistent with the terms of the order put in the mouth of the Commissary. Thus, when in May 1618 he presented to the Archduke Leopold of Austria a copy of his treatise on the tides, he was careful to accompany it with a statement that, since it was becoming to obey and believe in the decisions of the authorities, he wished any argument of his which rested on a Copernican basis to be regarded as a piece of poetry or a dream. It is manifest that this, though a literal compliance with the demands enforced against Copernicus by the Index Congregation, was no compliance at all with a precept not to teach or defend heliocentricism "in *any manner*, in writing or by word of mouth."

In 1624 Galileo wrote an elaborate answer to a letter against the Copernican system which had been addressed to him in 1616 by a certain *Franceso Ingoli*, who had in the meantime become secretary to the Congregation of the Propaganda. Here again he explicitly disclaims any intention of maintaining and holding as true that proposition which had been declared inconsistent with doctrines more majestic and authoritative than those of the natural sciences, and says that his object in writing was to show that, when he was engaged with astronomers and philosophers on this question, he was neither so blind of intellect nor so feeble in argument as to have taken the Copernican side for lack of having perceived, or understood, the instances produced by Ingoli. He then administers a severe rebuke to the unlucky secretary, telling him that, for every day he had devoted to the subject in hand, Copernicus had devoted a year, and that he must not lightly imagine himself able to overthrow so great a man as Copernicus, especially with such weapons as those he employs against him, which are objections of the commonest and most trivial description. After this he sets to work deliberately to tear the poor man's arguments to pieces one by one, for some forty pages or more. A copy of this production was forwarded by Galileo to Ciampoli, secretary to Pope Urban VIII. (who had succeeded Paul V. in 1624). It is difficult to suppose that Galileo, with a threat of proceedings in the Holy Office hanging over his head, would have dared to send to the Pope's secretary

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such a barefaced violation of the precept delivered him by the Commissary.

Still more decisive is the history, too long to be gone through in detail, of the negotiations which preceded the publication of the great "Dialogues." Galileo unhesitatingly placed in the hands of the licensing authority at Rome a work, the very title of which claimed the right of discussing the Doctrine of Copernicus, subject only to the limitations enacted by the Index Congregation, to whose decree the preface contains a formal adhesion. The work, after being subjected to minute and reiterated inspection, received the final approval of the licensers, and appeared with their Imprimaturs conspicuously displayed on its first page.

The conduct of the licensers is perfectly intelligible on the assumption that the decree of the Index alone guided their scrutiny, but utterly unaccountable on the opposite supposition. The only possible object which the Inquisition could have had in laying Galileo under the special personal restrictions detailed in the Commissary's precept was to bind him over to absolute silence about the condemned doctrine. It seems highly improbable that, in a case of such importance, an order like that of the Commissary should never have been notified to the Pope's licensers of books, should have become a dead letter, and within sixteen years been so utterly forgotten as to have been only "discovered" in a search among the archives of the Inquisition, instituted after the appearance of Galileo's "Dialogues."

The general character of the collateral evidence will be clear from the above summary. There remains only one more point of great importance for consideration, viz. the indications of genuineness or spuriousness afforded by the state of the muanscript trial-record itself as far as regards the disputed document. In his new edition, M. de l'Epinois has given a photo-lithographic facsimile of the minutes of February 25 and 26, and thus enabled other persons besides the exceedingly select few who have as yet been permitted to inspect the originals to form some idea of the evidence presented by their present condition. In the opinion of Wohlwill the result of a careful examination strikingly confirms his view. The first part of the minute of February 26, down to the Latin word translated "whole" in the version given above (pp. 260, 261), occupies the lower half of a left-hand page (fol. 378) of the manuscript; the remainder of it the adjoining right-hand page (fol. 379). The last two lines of fol. 378, in which the intervention of the Commissary is narrated, present, when examined under a magnifying glass, certain very peculiar phenomena which occur in no other part of the facsimile. A number of dots, and what look like remnants of older writing, are seen peeping out between the letters, and parts of letters, of the present text, and there is a blurred appearance about certain of the words, and notably about the all-important word "Commissary," which looks very much as if it were the result of

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writing over an erasure. Wohlwill's theory, which he supports by an investigation too technical for reproduction here, is that the minute of February 26 originally terminated at the foot of fol. 378 with a statement that Galileo had acquiesced under Bellarmine's admonition. The last two lines which contained it were then, he thinks, operated on by partial erasure and rewriting, including the use where possible of some of the old words or letters, so as to bring them into their present form. The fraud was then easily completed by writing the rest of the spurious minute on the next page, which, like many others in the manuscript, may very probably have been originally left blank. In doing this the genuine handwriting was carefully imitated, but, as Wohlwill confidently maintains, with but incomplete success.

It is proper to remark here that two of the three editors of the manuscript, Berti and v. Gebler, assert the genuineness of the disputed minute, while the third, De l'Epinois, has as yet expressed no opinion on the point. This fact affords some presumption against the view of Wohlwill, which was arrived at without personal access to the impugned document. A thorough re-examination of its contents by skilled and trustworthy palæographers alone offers a prospect of settling the question.

It is obvious that this new view of Galileo's trial involves the gravest charge against the Inquisition, that, viz., of having manufactured a spurious basis for a criminal indictment, and consummated a cruel wrong on the ground of an iniquitous fraud. If this charge is once established, it will be no easy matter to exonerate even the Cardinals of the supreme Congregation itself from complicity in the crime.

Clearly, therefore, the authorities of the Vatican will do well, unless they desire judgment to go by default, to produce additional documentary evidence, and bring this plain issue, so deeply affecting the good faith of the Holy Office in by far the most famous case ever decided at its bar, to a definitive settlement. The object of the few remaining lines devoted to this subject here will be to show that important unpublished evidence in the case unquestionably once existed, and presumably still exists, among the archives of the Inquisition.

It has been made abundantly clear by the most recent researches that the published trial-record is far from complete, that some of its documents are only copies or abstracts, and that others show plain marks of a later origin than the rest of the collection. The sentence and form of abjuration are both missing, as are also the opinions given by the consulting jurists of the Inquisition, though it is known from the Court's own statement that these opinions were actually obtained. Further, there is no report of the sittings held on the case by the supreme Congregation. The short references to decisions there arrived at are mere unauthenticated summaries. The original formally attested records of all these must once have formed part of the Inquisitional archives, and ought to be forthcoming now.

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On Hansen's Terms of Long Period in the Lunar Theory.

By E. Neison, Esq.

Recent investigations have shown that the theory of the terms of long period in the motion of the Moon is at present in a most unsatisfactory condition, so that the entire subject urgently requires further and more extended investigation. When Hansen announced that he had discovered the existence of two terms of long period in the motion of the Moon, due to the perturbations of *Venus*, it was thought that the last difficulty in the lunar theory had been overcome. Hansen's tables were found to completely represent the motion of the Moon for the period 1750–1850, and this was regarded as the most satisfactory evidence of the correctness of the theory on which they were based.

Unfortunately, in a most important point Hansen's tables do not really rest on his theoretical investigations. Purely empirical values were assigned by Hansen to the coefficients of the two important terms of long period which he had discovered to be produced by the planet *Venus*. Referring to these terms, Hansen says: "La détermination exacte de ces deux inégalités par la théorie est la chose la plus difficile que l'on rencontre dans la théorie du mouvement de la lune. J'ai cherché deux fois à déterminer leurs valeurs, et par des méthodes différentes, mais j'ai obtenu des résultats essentiellement différents l'un de l'autre." (*Monthly Notices* vol. xv., Nov. 1854.) For this reason, therefore, Hansen thought it best to give such empirical values to these terms as was necessary to bring theory and observation into accord during the interval 1750–1850. In this respect, therefore, Hansen's tables are decidedly unsatisfactory, and the fatal consequences of this course have swiftly overtaken his tables. The values for the coefficients of these two terms of long period, which Hansen obtained from his theoretical investigations, appear to have differed considerably from the empirical values employed by him in his tables. Hansen has stated that he intended undertaking a third investigation for the purpose of satisfactorily determining the theoretical value of these two coefficients. The results of this new investigation have never been made known, and it is doubtful if it was ever completed.

In 1858 Delaunay was led to take up the examination of these two terms of long period in the motion of the Moon, which had been discovered by Hansen to be produced by the action of *Venus*, for he remarks the lunar theory could not be left in the unsatisfactory condition mentioned by Hansen. Delaunay arrived at most important results; for whilst he confirmed from theory the value which Hansen had empirically assigned to the coefficient of the term of long period with the argument $(g-18g''+16g')$, using Hansen's notation (*Conn. des Temps*, 1862, App.), he found that the theoretical value of the coefficient of the other term, whose

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argument is $(8g''-13g')$, was so small that it might be regarded as insensible. (*Conn. des Temps*, 1863, App.)

This was a very serious matter; for if Delaunay's results were correct, they showed that there must be grave errors in the theoretical basis of Hansen's *Tables de la lune.* And if this were so, these tables could not continue for long to represent the place of the Moon. In 1870 Professor Newcomb pointed out that Hansen's tables failed apparently to represent the observations anterior to 1750, and he has lately confirmed this by a more extended investigation, in which he clearly shows that great discordances exist between the observed position of the Moon and the position assigned by Hansen's tables. What is of still more importance is the fact that of late the places of the Moon assigned by Hansen's tables have deviated very considerably from the observed place, and this deviation is rapidly increasing. It is evident, therefore, that although by giving empirical values to the coefficients of the two terms of long period which he had discovered to be produced by the perturbations of *Venus*, Hansen had been enabled to make his tables represent the observed place of the Moon during the interval 1750–1850; yet this had only been effected by means which had rendered his tables discordant prior and subsequent to this period.

It is certain, therefore, that there is some grave deficiency in Hansen's *Tables de la lune.* This fact strongly confirms the accuracy of Delaunay's conclusions with respect to the value of the terms of long period, but only to this extent, that Hansen's empirical values are wrong. For if Delaunay's values of these terms be adopted, and Hansen's tables be corrected, they still fail to represent the motion of the Moon, with this important difference, that they do not represent it for any period whatever. Either, therefore, both Hansen's and Delaunay's values are wrong, or there still remains to be discovered one or more important terms of long period in the theory of the Moon.

The determination of the value of the coefficients of the terms of long period in the motion of the Moon is an exceedingly laborious and difficult work. Not only are the analytical developments which are involved much more complex than in any other branch of the lunar theory, but they are of very great length. Generally the value of a term of long period is the difference between two large quantities of very nearly equal magnitude. These quantities are usually twenty to thirty times greater than the coefficient to be determined, and are in turn composed of a large number of terms of considerable magnitude and partially destroying each other. Consequently, the neglect of some comparatively insignificant term, or the slightest error of any kind, may be sufficient on one hand to quadruple the true value, or on the other hand to reduce it to zero. The numerical coefficients which are involved in these terms must be carried to a point of extreme accuracy, for I have found the value of an important term to be vitiated from my having carried these numerical coefficients only

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to the fifth decimal place, instead of to the seventh decimal place, although accuracy to the third decimal place appeared to be ample. It is true that these difficulties are present in a very mitigated form in the two terms of long period which were discovered by Hansen. Yet, as the remarks quoted from Hansen show, their determination was found by him to be the most difficult work he had encountered in the lunar theory. Moreover, Poisson and Pontécoulant were unsuccessful in determining the value of one of these two terms, and Delaunay himself failed in his first attempt to calculate the value of the other. All three failures were due to their having omitted some apparently insignificant factor, and so obtaining a value far less than the truth. It was probably on account of the known difficulty of the problem that Delaunay's investigations do not seem to have attracted the attention that was merited by the importance of their subject.

It is now generally admitted that the value which Hansen assigned to the coefficient of the term of long period whose argument he denoted by $(8g''-13g')$ is far greater than its true value, and that we must abandon this second term of long period which is produced by the planet *Venus*. Delaunay's conclusion that its coefficient was very small has been completely confirmed by the entirely independent researches of Professor Newcomb. My own results are in accordance with those of Delaunay, and I have sufficiently verified the analytical developments employed by Delaunay in his memoir to satisfy myself that they are not vitiated by one of those accidental errors from which even the most careful work of the finest mathematicians is not necessarily exempt. It may be regarded as certain, therefore, that Delaunay's investigations are free from any serious error. Yet I understand that Hansen would never admit that Delaunay's researches established the inaccuracy of his own earlier investigations, although I cannot learn that Hansen ever definitely stated the grounds of his objection. It has always appeared to me that Hansen must have had some good reason for his objection, but that he was waiting until he had completed his third investigation of this difficult problem.

My own investigations, whilst they completely confirm the accuracy of Delaunay's result, have led me to the unexpected conclusion that Hansen may be correct after all. Are Delaunay's investigations complete?

I do not refer to the terms depending on the square of the mass of *Venus*, and to which Professor Newcomb has drawn attention in his remarkable paper in the *American Journal of Science* for November 1877, because all investigators have hitherto neglected them, so that it cannot be these terms which produce the discrepancy between Delaunay and Hansen. But I doubt if Delaunay's investigations are really complete as far as terms depending on the first power of the mass of *Venus*, and it appears to me that there is strong evidence that it was from these very

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terms, omitted by Delaunay, that Hansen obtained the large coefficient of the second *Venus* term—that with the argument $(8g''-13g')$.

In the *Memoirs of the Royal Astronomical Society*, vol. xvi., Hansen has published the only existing details of his method of computing these inequalities, and on page 476 there occurs the following passage. Speaking of the term whose argument is denoted by him by $(g-18g''+16g')$, and whose value as far as the first power of the disturbing forces he had just computed, Hansen says : " Cependant le cube des forces perturbatrices, ou plutôt le produit du carré et du cube de la force perturbatrice du soleil par la force perturbatrice de *Venus*, ajoute une quantité sensible à ce coefficient, et rend en même temps sensible le coefficient de l'argument $(8g''-13g')$, qui, en tenant seulement compte de la première puissance de la force perturbatrice, serait insensible dans le mouvement de la lune." Delaunay appears to me to have entirely overlooked the real bearing of this important statement, for I think it clearly defines from whence Hansen obtained the large value for the coefficient of the argument $(8g''-13g')$, which was found to be insensible by Delaunay.

The direct action of the planet *Venus* in producing perturbations in the motion of the Moon may be analytically represented by a particular disturbing function made to form part of the complete disturbing function which represents the action of every disturbing body on the Moon. This particular disturbing function depends on the undisturbed geocentric coordinates of the Moon and *Venus* and on the complete perturbations of the Moon. By neglecting the second and higher powers of the mass of *Venus*, this particular disturbing function is developed in terms of the heliocentric elliptic coordinates of *Venus* and the Earth, the geocentric elliptic coordinates of the Moon, and of the different powers of the perturbations in the motion of the Moon which are produced by the disturbing action of the Sun. Delaunay has exclusively confined himself to this portion of the complete disturbing force when finding the direct action of *Venus* upon the Moon, and, as far as the first power of the disturbing forces, it is evident that this is the only portion which need be considered. As far as the first power of the disturbing forces, Delaunay finds a value for the term $(g-18g''+16g')$ which is sensibly identical with that found by Hansen under like conditions. For the term $(8g''-13g')$ under these conditions Delaunay finds its coefficient is insensible. In this also he agrees with Hansen, as the paragraph which has been quoted will show. To this order Delaunay's investigations must be complete, and to this order both Hansen and Delaunay have obtained identical results.

When Delaunay takes into consideration the effect of the different powers of the perturbations in the motion of the Moon arising from the disturbing action of the Sun, in all cases he considers the effect of the first power of these perturbations, and in one particular term he includes the second power of these pertur-

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bations. Delaunay shows that these terms do not materially augment the value of the two terms of long period. Granting that these results are perfectly accurate, and there can be no real doubt about their accuracy, is there any discrepancy between them and those which Hansen obtained? It cannot be said that there is; for in the paragraph which has been quoted, Hansen explicitly states that the coefficient of the term $(8g''-13g')$ is insensible unless there be included the terms depending on the product of the first power of the disturbing force of *Venus* with the square and cube of the disturbing force of the Sun. But Delaunay has entirely omitted the terms of this order in his investigations. Delaunay himself does not seem to have noticed this discrepancy, partly because he seems to call the first power of the disturbing force of *Venus* also the first power of the disturbing force of the Sun. And his investigations being performed with the aid of his new and elegant method of attacking the lunar theory, in this portion they are not easily followed by anyone not perfectly familiar with this most laborious method. Had this not been so, it is probable that attention would have been directed before now to this discrepancy.

There is little difficulty in ascertaining what were probably Delaunay's reasons for omitting these terms. It is obvious that the terms neglected will be smaller than those which he has retained, and which depend on the first power of the disturbing force of the Sun. But these last are very small, so that there could be no valid reason for including the others which were still smaller. Delaunay was therefore perfectly justified in omitting these terms. But this very circumstance is enough to suggest the inquiry whether Hansen was really referring to these terms, or whether, in his remarks, he does not refer to some entirely distinct class of terms whose existence has been overlooked by Delaunay. Is there, then, a second class of terms to which Hansen's remarks may really apply? If there is, it at once affords a clue to an explanation of the discrepancy between the results obtained by Hansen and Delaunay, for the latter has confined himself entirely to the one particular class of terms which has been described above.

It appears that there is another and still more direct way in which the disturbing action of the Sun will influence the perturbations in the motion of the Moon produced by the disturbing action of a planet.

Let ρ = geocentric distance of the disturbing body; H = geocentric angular distance between the disturbing body and the Moon, r, v = geocentric radius vector and longitude of the Moon; then the disturbing function representing the action of this body upon the Moon will be

$$\mathrm{R}_1 = \mathrm{M}_1\left\{\frac{r^2}{\rho^3}\left(\frac{3}{2}\cos^2\mathrm{H}-\frac{1}{2}\right)+\cdots\quad\right\},$$

where M denotes the mass of the disturbing body. Suppose the

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inclination of the orbit of the Moon and that of the disturbing body to be so small that the product or square of the inclinations may be neglected. Let r_1, v_1 denote the heliocentric radius vector and longitude of the disturbing body, and r_o, v_o the heliocentric radius vector and longitude of the Earth. Then

$$\rho = \{r_1^2 + r_o^2 - 2r_1 r \ \cos(v_o - v_1)\}^{\frac{1}{2}},$$

$$\cos \mathrm{H} = \left\{\frac{r_1}{\rho}\cos(v - v_1) - \frac{r_o}{\rho}\cos(v - v_o)\right\}.$$

Substituting this value of cos H in the expression for R_1, it becomes

$$\mathrm{R}_1 = \mathrm{M}_1\left\{\frac{1}{4}\cdot\frac{r^2}{\rho^3} + \frac{3}{4}\frac{r^2}{\rho^5}[r_1^2\cos(2v - 2v_1) + r_o^2\cos(2v - 2v_o) - 2r_1 r_o\cos(2v - v_1 - v_o)]\right\}.$$

This will be the form of the disturbing function which expresses the direct action of a planet upon the Moon, and it is to this portion of the complete disturbing function that Delaunay has entirely confined his attention. The disturbing function which expresses the disturbing action of the Sun upon the Moon will be found by making $r_1 = 0$ in the above. It will be

$$\mathrm{R}_o = \mathrm{M}_o\left\{\frac{1}{4}\cdot\frac{r^2}{r_o^3} + \frac{3}{4}\frac{r^2}{r_o^3}\cos(2v - 2v_o)\right\}.$$

Let the complete disturbing function be denoted by R, and suppose it to be developed in a series of cosines in the usual manner,

$$\mathrm{R} = \mathrm{R}_o + \mathrm{R}_1 = \Sigma\mathrm{R}_o\cos\beta_o + \Sigma\mathrm{R}_1\cos\beta_1 = \Sigma\mathrm{R}\cos\beta.$$

Throughout the subscript $_o$ will be employed to denote quantities arising from the disturbing action of the Sun, and the subscript $_1$ to denote all quantities arising directly or indirectly from the disturbing action of the planet. Let R^o denote the value of R when the perturbations of the coordinates are neglected; it is thus necessary to develop R in terms of R^o and the perturbations. Both R_o and R_1 involve the lunar coordinates in identically the same manner, so that their differentials with respect to these coordinates will possess the same identity. Direct differentiation gives

$$\left(\frac{1}{r}\right)^n \frac{d^n\mathrm{R}^o_1}{d\left(\frac{1}{r}\right)^n} = (-1)^n.\Gamma(n+2).\Sigma\mathrm{R}^o\cos\beta.$$

$$\frac{d^n\mathrm{R}}{dv^n} = 2^n.\cos n\pi.\Sigma\mathrm{R}^o\cos\beta - 2^n.\sin n\pi.\Sigma\mathrm{R}^o\sin\beta.$$

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The differential with regard to v vanishes for those terms in the disturbing function not arising from the part of the disturbing function involving v, that is for all terms whose arguments are independent of the angular distance of the Moon and the Sun or other disturbing body. For the present purpose there is no need to separate these two portions, as the distinction can be remembered.

Suppose the complete perturbations of the Moon to be expressed by

$$r\delta\frac{1}{r} = \Sigma a_o \cos\alpha_o + \Sigma a_1 \cos\alpha_1, \qquad \delta v = \Sigma b_o \sin\alpha_o + \Sigma b_1 \sin\alpha_1.$$

To develop R let

$$D^o{}_r = \left(r\delta_o\frac{1}{r}\right)\left(\frac{1}{r}\cdot\frac{d}{d\frac{1}{r}}\right), \qquad D^1{}_r = \left(r\delta_1\frac{1}{r}\right)\left(\frac{1}{r}\cdot\frac{d}{d\frac{1}{r}}\right),$$

$$D^o{}_v = (\delta_o v)\left(\frac{d}{dv}\right), \qquad D^1{}_v = (\delta_1 v)\left(\frac{d}{dv}\right),$$

then a known symbolical expansion gives

$$\begin{aligned}
R &= R^o + \delta R \\
&= \{D^o{}_r + D^1{}_r + D^o{}_v + D^1{}_v\} R^o \\
&= \{\epsilon^{D^1{}_r} . \epsilon^{D^1{}_v}\}\{\epsilon^{D^o{}_r} . \epsilon^{D^o{}_v}\} R^o \\
&= \{(1 + D^1{}_r + \cdots\ )(1 + D^1{}_v + \cdots\ )\} \times \\
&\qquad \{(1 + D^o{}_r + \frac{1}{\lfloor 2}\cdot(D^o{}_r)^2 + \cdots\ )(1 + D^o{}_v + \frac{1}{\lfloor 2}(D^1{}_v)^2 + \cdots\ )\} R^o.
\end{aligned}$$

Replace D and its powers and products by their values and perform the operations, then the expression for R becomes

$$R = \Sigma R^o\{[\cos\beta - 2(a_o\cos\alpha_o)\cos\beta - 2(b_o\sin\alpha_o)\sin\beta + 3(a_o\cos\alpha_o)^2\cos\beta + 4(a_o\cos\alpha_o)(b_o\sin\alpha_o)\sin\beta - 2(b_o\sin\alpha_o)^2\cos\beta + \cdots$$

$$= \overset{\pm}{\Sigma} A_o\cos(\beta\pm\alpha_o)] \qquad \}$$

$$+ \Sigma R^o\{[\cos\beta - 3(a_o\cos\alpha_o)\cos\beta - 2(b_o\sin\alpha_o)\sin\beta + 6(a_o\cos\alpha_o)^2\cos\beta + 6(a_o\cos\alpha_o)(b_o\sin\alpha_o)\sin\beta - 2(b_o\sin\alpha_o)^2\cos\beta + \cdots$$

$$= \overset{\pm}{\Sigma} B_o\cos(\beta\pm\alpha_o)]\times[-2a_1\cos\alpha_1]\}$$

$$+ \Sigma R^o\{[\sin\beta - 2(a_o\cos\alpha_o)\sin\beta + 2(b_o\sin\alpha_o)\cos\beta + 3(a_o\cos\alpha_o)^2\sin\beta - 4(a_o\cos\alpha_o)(b_o\sin\alpha_o)\cos\beta - 2(b_o\sin\alpha_o)^2\sin\beta + \cdots$$

$$= \overset{\pm}{\Sigma} A_o\cos(\beta\pm\alpha_o)]\times[-2b_1\sin\alpha_1]\}$$

$$+ \cdots$$

Let $a_oa_ob_o$ denote the coefficient of the argument α_o in the function $(a_o\cos\alpha_o)^2(b_o\sin\alpha_o)$ and similarly with other terms; then

$$\overset{\pm}{\Sigma} A_o = 1 - a_o \pm b_o + \frac{3}{2}a_oa_o \mp 2a_ob_o - b_ob_o + \cdots \qquad \overset{\pm}{\Sigma} B_o = 1 - \frac{3}{2}a_o \pm b_o + 3a_oa_o \mp 3a_ob_o - b_ob_o + \cdots$$

and the disturbing function can be written

$$R = \Sigma R^o_1(\overset{\pm}{\Sigma} A_o)\cos(\beta_1\pm\alpha_o) + \Sigma R^o_o\{(\overset{\pm}{\Sigma} A_o)\cos(\beta_o\pm\alpha_o) + [\overset{\pm}{\Sigma} B_o\cos(\beta_o\pm\alpha_o)](-2a_1\cos\alpha_1) + [\overset{\pm}{\Sigma} A_o\sin(\beta_o\pm\alpha_o)](-2b_1\sin\alpha_1) + \cdots \quad\}.$$

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In these expressions all the upper and all the lower signs are to be taken together, and the terms depending on the square of the mass of the disturbing planet have not been written down. There is obviously not the slightest difficulty in at once writing down every term to any desired order of the disturbing forces. When β is an argument which is independent of the angular distances between the Moon and disturbing body, the corresponding coefficient b must be put equal to zero in the coefficients ΣA_0 and ΣB_0. This is the form of the disturbing function which I have employed in my investigations on the terms of long period in the motion of the Moon; and it is particularly convenient, from the portions depending on the solar perturbations being kept distinct from that depending on the disturbing planet, so that they will be identical for every term of this kind in the theory of the Moon.

The first portion of this expression is the development of the disturbing function which has been denoted by R_1, and which represents the direct action of the planet upon the motion of the Moon, and upon the perturbations in this motion arising from the disturbing action of the Sun. It is this portion alone of the above expression which has been considered by Delaunay. The first term in the second portion is the expression of the part of the disturbing function due to the disturbing action of the Sun, and from which are derived the ordinary perturbations of the lunar theory. The second and third terms of this second part arise from substituting the complete perturbations of the Moon in the value of the ordinary disturbing function representing the action of the Sun upon the motion of the Moon. They really represent the portion of the disturbing function which expresses the action of the Sun upon the perturbations produced by the disturbing action of the planet upon the motion of the Moon. They yield the new class of terms to which attention has been directed.

The theory of the variation of parameters gives the following equation for determining the terms of long period in the mean motion of the Moon:—

$$\frac{d^2\delta n}{dt^2} = -3an^2\left(\frac{dR}{dE}\right),$$

where a is the mean distance, and E the longitude of the epoch. Let f be the arithmetical coefficient of E in the argument which has been denoted by β; then

$$\left(\frac{dR}{dE}\right) = \Sigma\,(-f)\,R\sin\beta.$$

The complete development of this expression can be obtained from that of R by writing $\beta-\frac{\pi}{2}$ for the arbitrary argument β. It will be therefore

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$$\left(\frac{d\mathrm{R}}{d\mathrm{E}}\right) = \Sigma\,(-f).\,\mathrm{R}^o_1\,(\overset{\pm}{\Sigma}\mathrm{A}_o)\sin(\beta_1 \pm \alpha_o) + \Sigma\,(-f)\,\mathrm{R}^o_o\{(\overset{\pm}{\Sigma}\mathrm{A}_o)\sin(\beta_o \pm \alpha_o)$$

$$+ [\overset{\pm}{\Sigma}\mathrm{B}_o \sin(\beta_o \pm \alpha_o)]\,(-2\mathrm{a}_1 \cos \alpha_1) + [\overset{\pm}{\Sigma}\mathrm{A}_o \cos(\beta_o \pm \alpha_o)]\,(-2\mathrm{b}_1 \sin \alpha_1) + \cdots \}$$

It remains to be shown that the second portion of this expression will give rise to terms of long period with the same argument as those which can be obtained from the first portion, and which have alone been considered by Delaunay.

Let Z cos ζ denote the complete value in $r\delta\,\frac{1}{r}$ of the term of long period which is derived from the first part of the above expression, and Z^1 sin ζ its complete value in δv. These will then form particular forms of the general terms $\mathrm{a}_1 \cos \alpha_1$ and $\mathrm{b}_1 \sin \alpha_1$. The function which forms the coefficient of the term $(-2\mathrm{b}_1 \sin \alpha_1)$ will have a constant portion of the order of the square and cube of the disturbing force of the Sun, and consisting of the coefficient of every pair of arguments in the series of arguments expressed by β_o and α_o which make $\beta_o = \alpha_o$.

This constant portion will be of the order m^4, and may be denoted by the term $\Sigma\,(\mathrm{R}_o{}^o)_x(\Sigma\mathrm{A}_o)_x$. Substituting these two values, then, as far as the term with the argument ζ, there will exist the term

$$\left(\frac{d\mathrm{R}}{d\mathrm{E}}\right) = \{\Sigma\,(\mathrm{R}^o_o)_x\cdot(\Sigma\mathrm{A}_o)_x\}\,(-2\mathrm{Z}^1).\sin\zeta.$$

In a similar manner other terms will arise from this second part of the expression having the argument ζ and with coefficients containing Z and Z^1 multiplied by quantities of the order of the square and cube of the disturbing force of the Sun. Now Z and Z^1 are of the order of the disturbing force of the planet. This new class of terms must be described, therefore, as arising from the product of the square and cube of the disturbing force of the Sun with the disturbing force of the planet.

I think it is unquestionable that it is to these terms that Hansen refers in the paragraph which I have quoted from his memoir, and that it is from these terms that Hansen has derived the large value of the coefficient of the term of long period with the argument $(8g'' - 13g')$. For it is to these terms, and to these terms alone, that Hansen's words naturally apply. As these terms have been entirely neglected by Delaunay, this view would at once afford a possible explanation of the discrepancy between Hansen and Delaunay, and show at once why Hansen would not admit the correctness of the conclusions which were obtained by Delaunay. For these terms, depending on the ordinary perturbations arising from the direct action of *Venus* multiplied by the second power of the disturbing force of the Sun, will appear in the perturbations of the mean motion divided by the square of the very small coefficient of the time. They can very easily be far greater than the perturbations arising from

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the direct action of *Venus*, which have been alone considered by Delaunay (the indirect effect of *Venus* through the agency of the perturbations of the Earth being disregarded). I think, therefore, it is almost certain that it is in this way that the discrepancy between Hansen and Delaunay has arisen.

It does not necessarily follow that the theoretical results which were obtained by Hansen are necessarily correct, although I have no doubt that it was from this second class of terms that Hansen derived the large value of the coefficient of the doubtful *Venus* term, and that consequently Delaunay's investigations do not really invalidate Hansen's conclusions. From his own remarks, it is evident that Hansen found very great difficulty in determining the theoretical value of these terms, a work of immense labour and difficulty by any of the ordinary methods. It is very possible that both of his results may have been vitiated by some error, just as one of the two conflicting results must have been wrong. The computations being extremely long, it is not unlikely that some error may have crept in; and it must be remembered that Delaunay, by neglecting these terms, has omitted the most difficult portion of the computation. It may be that these terms are altogether insensible. It is still more probable that the largest portion of them are mutually destructive, and in this manner vanish. I would urge merely that, until they have been shown to be insensible or to disappear, the theory of these two terms of long period must be considered to be imperfect.

These terms are of the third and fourth order of the disturbing forces, and it may be that it is possible to show by some established analytical principle, such as the invariability of the semi-axis major, that these terms must be mutually destructive. It is possible that Professor Newcomb may have investigated this point in his unpublished researches, and have thus shown that these terms do not affect the terms of long period in the lunar theory. I should be glad to learn whether this is so, for the past history of the lunar theory shows more than one instance where such principles have been overlooked, or else erroneously imagined to exist. To be sure that these terms may be safely neglected when determining the value of the coefficients of terms of long period similar to Hansen's, it must be shown that they vanish as far as the order m^7. To effect this by a direct computation would be a difficult and lengthy task. To show that they may be neglected in the case of all the terms of long period to which I have directed attention, it would be necessary to prove that they vanish to the order m^{10}. To effect this by direct computation would be an enormous work, and one which could not be carried out without pushing the lunar theory to a higher order than has ever yet been done.

I have endeavoured, therefore, to determine the value of these terms by a different method. In many portions of the lunar theory there arise different classes of important terms which in

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the end destroy each other and so vanish. By developing the disturbing function in a general form it is possible, by an application of the method of the variation of arbitrary constants, to ascertain what terms destroy each other without its being necessary to determine the value of the terms in question. Thus if R be a function of the form $r \begin{matrix}\sin\\ \cos\end{matrix} \beta$, where r is constant and only β variable, as far as constant terms the function

$$R\int R\, dt = 0.$$

It can be further shown that in this expression every term is multiplied by the coefficient of the time in its argument. By similar considerations to these the work can be much reduced.

To show that the class of terms which have been referred to vanish to the order m^{10}, it would be necessary to show that they are mutually destructive to the fifth power of the disturbing forces, whereas no general development of the theory of the variation of constants seems to have been pushed beyond the terms of the second power of the disturbing forces. By a transformation I have pushed the development as far as the third power of the disturbing forces, and in part, though at present not completely, to the fourth power.

By this means it can be shown that the terms of the order m^4 and m^5 vanish from the perturbations in the mean motion, or before integration are multiplied by the small coefficient of the time in their argument. The same is true of most if not all of the terms arising from the second power of the disturbing force of the Sun and of the order m^6 and upwards. The same does not appear to hold of the third power of the disturbing force of the Sun or of the fourth power of the entire disturbing forces. I have not yet completed this last portion, but terms of the order m^6 appear to actually remain undestroyed. This result is confirmatory of the results obtained by direct computation in a very different manner.

If, then, there remain terms of the order m^6, it is quite possible that Hansen's theoretical values of the two terms of long period arising from *Venus* may be accurate.

On the Telescopic Observations of the Transit of Venus 1874, *made in the Expedition of the British Government, and on the Conclusions to be deduced from those Observations.* By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope.

I received by the last mail a copy of the Parliamentary Report whose title heads this Paper. This report contains the details of the observations of contact made by the observers of the British